



2

*Reprinted from the New York Medical Journal for  
June 21, 1913.*

## THE CANCER RESEARCH INSTITUTE.\*

By J. G. ADAMI, M. D., Sc. D., LL. D., F.R.S.,  
Montreal,

Professor of Pathology, McGill University.

I have been asked to speak upon the subject of the Cancer Research Laboratory or Institute, and find myself in the uncomfortable position, despite the best of intentions, of being able only to utter mere commonplaces. For, the more I have thought over the matter, the more it has impressed itself upon me that the recommendations that I can make are such as are already being carried out at the present moment in this city of New York; my conclusions must therefore be familiar to all here present who are interested in the subject.

The primary, as it is the ultimate, object of cancer research, is, let me emphasize, the discovery, not of the cause of malignant new growth, but of the means whereby humanity may be protected against this, its most awful scourge. Keeping this in mind, it will be seen that there are two methods of attack, both logical, both scientific, and it is for us to determine which promises the better results, the fuller and richer fruit. These I may term the direct and the indirect methods. Of the value of the former Ehrlich has recently afforded us a unique and triumphant demonstration. It is the method of experimental chemotherapy, and consists in this, that having first selected some drug or substance of known chemical composition, seen to possess a definite although imperfect influence upon the course of a particular disease, the investigator, with his corps of chemical assistants, proceeds to elaborate a succession of compounds of this primary

---

\*An address delivered at a stated meeting of the New York Academy of Medicine, May 15, 1913.

substance with various radicals, and with his pathological assistants tests the relative value of these new compounds *seriatim* upon animals of the laboratory inoculated with the disease in question.

The research may be most laborious; hundreds of compounds may have to be devised, tested, and rejected, but it is a reasonable expectation that gradually in the course of all these trials certain groupings or radicals will be encountered presenting definite and specific action upon one or other phase of the particular morbid process, and that eventually some permutation or combination of such substance or substances will be discovered which will effectively destroy the causative agent within the tissues, or arrest the morbid process without coincident grave injury to the tissues of the individual. As all know, this is the method which is being followed by Wassermann and his coadjutors at the present moment at the Berlin Gesundheitsamt, and that with a certain measure of reported success in regard to cancer of the animals of the laboratory. It is a method which does not wait for preliminary studies upon the etiology and life history of cancerous and other new growths; it is a purely therapeutic method, even if at the same time purely experimental.

But it is by means of the other or indirect method that scientific medicine in our generation has scored its most notable triumphs. It is the method based upon the old Baconian aphorism. "*Vere scire est per causas scire*"; it is the etiological method. It may be laid down with a confidence so absolute that it becomes a truism that the extraordinary advances that have been made in the treatment and control of tuberculosis, diphtheria, and other infections during the last thirty years date from the moment at which the causative agents of these diseases became determined, or, failing that (as in the case of yellow fever), date from the time when, employing rigid analogy, the nature of the disease became realized; that is to

say, the detection of the cause of one disease threw light upon the mode of prevention of an allied disease. Nay, more, without any depreciation of Ehrlich's most notable achievement, and without minimizing our admiration for the prodigious labor that led up to the discovery of "606," we must, I think, admit that even in Ehrlich's hands the direct method could not have succeeded but for the previous discovery of the causative agent of syphilis. It was the discovery of trypanosomes and treponemes, the fact that they can be detected in the blood or in the tissues, that made it possible to determine surely the influence of one after the other of the long series of compounds that was elaborated and tested. It is noteworthy that our knowledge of the therapeutic action of Jesuit's bark, or quinine, in relationship to malaria, dates back to the seventeenth century, but, notwithstanding this knowledge, it has only been in our own generation, only since Laveran's discovery of the malarial parasite, and Ross's elucidation of its life history, that we have been able to make the Isthmus of Panama and the New Jersey coast decently habitable.

Thus, while admitting that what may be termed the "Ehrlich method" pure and simple may give good results, I have grave doubts whether, applied to cancer, of which admittedly the causation is still undetermined, it can promise us anything in the nature of a full control of the situation. The bulk of scientific workers must, I imagine, be with me when I lay down that the indirect method of attack is that which should be selected, and that the ideal institute for cancer research must approach the solution of the problem by investigation into the nature of the process, into the nature of normal cell growth, into the life history of malignant growths, and so into the cause or causes of neoplasia. It is this course of action that promises most for the benefit of humanity.

Admit, then, that an institute along the latter

lines is the better. Admit also that, despite the labors of a generation of multitudinous investigators, the cause of cancer is still undetermined. How are we to proceed? This, I think, will be accepted by all sensible men that the frontal attack offers little promise of success. It has been attempted too often with constant repulse. In other words, the energies of the institute and its staff must not be devoted to immediate endeavors to determine the cause. It is evident that the position has to be invested from all sides and thoroughly undermined; this course alone would seem to favor eventual success. And, to continue the metaphor, the question for us to determine is whether the ideal institute should regard itself as a single corps of the investing army, and as such devote itself to carrying forward the siege works against one particular aspect of the position; or on the contrary, whether it should be of the nature of headquarters for the staff, from which qualified men can be sent out to direct operations against the besieged, some on this side, others on that. There is much to be said in favor of each mode of procedure. The ultimate decision must depend upon the size of the war chest, as also I should add, upon the officer chosen for command. A man may be a superb regimental officer, but as a general in command may be a notable failure. Nevertheless, however brilliant the abilities of the commander, if the amount of money for the campaign placed at his disposal is so small that he can have but few men under him, it stands, I think, to reason that he is likely to accomplish more by concentrating their work and their efforts upon one particular line of attack than by frittering their energies in several directions. And here the director of the institute is exposed to a dangerous temptation. Those who have provided the war chest desire to see results; it requires no little strength of mind to concentrate upon one particular line of attack with the full knowledge that it may very possibly turn out to be futile; there is



a distinct temptation in the circumstances to indulge in warfare of a more guerrilla type, knowing that if there are frequent repulses, there may from time to time be brilliant inroads upon the enemy's position. Now cancer research, to be well conducted, necessitates heavy expenditure, expenditure extending over long years; so heavy that, in general, I would lay down that the institute is most likely to be of service if it takes up one or at most two lines of work. The Imperial Institute for Cancer Research in London, for instance, may not have discovered the cause or mode of arrest of cancer, but it has done most valuable work by its accumulation of statistics of cancer incidence in different parts of the world, and among the lower animals. Thereby it has undoubtedly cleared the path and prepared the way for a more accurate understanding of the problem before us. Ehrlich's laboratory, devoting itself as it did for so many years, to a study of transplantation of tumors in the lower animals, has not, it is true, discovered the source of those tumors, but undoubtedly has added most valuable knowledge of the life history of malignant growths; confirmed and extended as it has been by the concentrated work accomplished by the British Imperial Institute. At St. Louis good work is being accomplished by studies upon the biology of the cell and its reactions to various stimuli. Nor has the frontal attack directed from the Gratwick Laboratory been other than most valuable. If Doctor Gaylord and his associates have failed to discover the causative agent of cancer, they have done yeoman service by the method of exclusion; they, too, have cleared the path. The same may be said with regard to the work of the institute under Professor Czerny at Heidelberg; if it has not discovered a cure for cancer it has done work of the highest value in testing and excluding reputed remedial methods. All must admit that through the work of these institutes, which tend to concentrate their energies upon the elucidation of particular

problems, and, let me add with full appreciation, through the work of those other institutes and agencies like the Middlesex Hospital Cancer Research Laboratory, and the Harvard Cancer Commission, which have indulged in a more guerrilla warfare, we to-day have a much fuller knowledge of very many aspects of the problem than we possessed ten years ago; we are in a position to direct our endeavors far more methodically and more serviceably than at any previous period.

If, however, there be abundant funds, the ideal institute in a centre such as this, a centre possessing also abundant expert research workers in various branches of biology, may profitably, and indeed preferably, embrace the wider scheme, and may gather to it experts of all orders to make a combined attack from many sides. In such a great centre as New York, what I believe to be the plan outlined by Columbia University, under the Crocker bequest, and arrived at independently, I believe, by the Cornell authorities, appears to me to be admirable. I am only conversant with the bald outlines of the scheme; it is possible, therefore, that what I am about to say may not in its details fully conform to what is here being carried out. But briefly, my ideal for a cancer research institute in a great and wealthy city is a combination of an institute and what at Harvard they term a "commission," such as shall utilize to the fullest all possible means of attack. There must, I would lay down, be a central institute or administration building. This need not, it is true, be of great size, but should contain a library where should be collected all modern literature bearing upon cancer research, a director's office, and a suite of laboratories and animal rooms for the use of those making investigations in association with, or under, the immediate direction of the head of the institute. And, whether in the library or elsewhere, there should be a conference chamber for the periodical gathering together of workers to report upon and discuss the researches

